is due to the error introduced into the calculations by referring the impurity to the hydrogen. But, whether this explanation be accepted or not, it is clear that the three lines drawn through the points representing the three series of ratios ought to meet at a point on the horizontal line of zero impurity, for the errors, whether due to chemical action or to calculation, would disappear with the cause that produced them. Hence, if no other source of error is present, the true ratio may be found by taking the most probable point of intersection of the three lines on the horizontal line of zero impurity. It is not easy to determine exactly the position of this point: it probably lies between the values 1·996 and 1·998, and the true ratio may perhaps be taken as 1·997 Dr. Scott adopts the ratio 1·994, but this appears to me to be certainly a little too low.

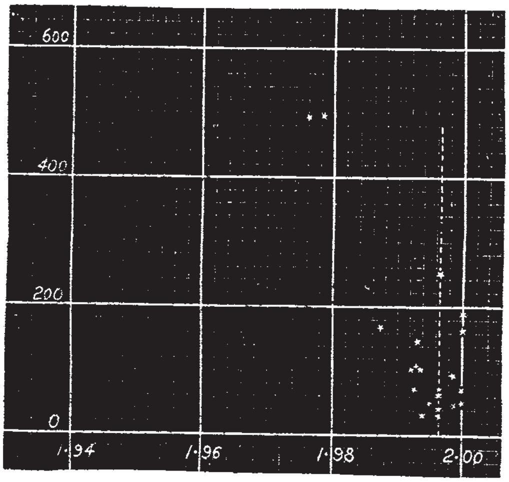


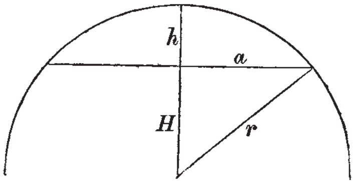
Fig. 3.

Prof. Thorpe shows that the atomic weight of oxygen, calculated from Regnault's densities of oxygen and hydrogen, corrected by Prof. Le Conte, and Dr. Scott's ratio (1·994) for the combining volumes, is 16·009. The ratio 1·997 would make the atomic weight O = 15·985. SYDNEY YOUNG.

University College, Bristol.

The Fog Bow and Ulloa's Ring.

IN the summer of 1875, I made a tour of inspection to our meteorological stations in the surveying-steamer *Hansteen*, Capt. M. Petersen, R.N. During the morning hours of August 7, I was on shore at Gandfjord, on the south side of the Varangerfjord, and measured the height of some terraces there. At 1h. 10m. p.m. we took serial temperatures in the Gandfjord with the deep-sea thermometer. The weather was calm, and a dense fog prevailed. The temperature of the air was 12° C. Leaving the Gandfjord we proceeded northwards. The dense fog continued. At once the fog began to be lighter and the sun to shine through, and a few minutes afterwards we were out of the fog, which was standing as a white wall in the south-west. In the moment the sun appeared, but before we were quite clear of the fog, I saw in the north-east a bow having the shape of a rainbow, but quite white, projected on the fog. With a sextant I measured its amplitude, or the chord along the horizon, and the height of the summit above the horizon—in both cases the middle between the outer and inner edge of the bow. The horizon not being distinctly visible, it is probable that the measures taken do not exactly refer to the true horizon, nor is it certain that the height of the summit was taken from the same horizontal plane in which the amplitude was measured. By the captain's reckoning, the apparent ship's time, at the moment of observation, was 2h. 40m., and the latitude 70° 1′. From these data, and the declination of the sun, I computed the azimuth of the sun at south 46° 5′ west, and its apparent altitude at 31° 12′. Supposing, as the results of the several computations tend to indicate, that the white bow is circular, and has its centre in the anthelic point, we may calculate the angular radius of the bow by three different methods.



Let h represent the height of the summit of the bow above the horizon, a half the amplitude or chord along the horizon, H the dip of the centre of the bow below the horizon, supposed to equal the altitude of the sun, and r the angular radius of the bow. Then we have—

$$r = \mathrm{H} + h \quad \ldots\ldots\ldots\ldots \quad (1)$$

$$dr = d\mathrm{H} + dh \quad \ldots\ldots\ldots\ldots \quad (1')$$

$$\cos r = \cos a \cos \mathrm{H} \quad \ldots\ldots\ldots\ldots \quad (2)$$

$$dr = \frac{\tan a}{\tan r}\,da + \frac{\tan \mathrm{H}}{\tan r}\,d\mathrm{H} \quad \ldots\ldots\ldots \quad (2')$$

$$\cos r = \cos a \cos (r - h)$$

$$\tan r = \frac{1 - \cos a \cos h}{\cos a \sin h}, \text{ or putting}$$

$$\cos a \cos h = \sin^2 \mathrm{M}$$

$$\tan r = \cot^2 \mathrm{M} \cot h \quad \ldots\ldots\ldots \quad (3)$$

$$dr = \cos (r - h) \frac{\cos r}{\sin h} \tan a \, da - \sin (r - h) \frac{\cos r}{\sin h} dh \quad (3')$$

The observations gave $2a = 49^\circ$, $a = 24^\circ\ 30'$, and $h = 7^\circ$ (or a little more).

From (1) we have $r = 31^\circ\ 12' + 7^\circ = 38^\circ\ 12'$.

Putting $d\mathrm{H} = \pm 2'$, $dh = \pm 15'$, we have by (1′)

$$dr = \pm \sqrt{2^2 + 15^2} = \pm 15'{\cdot}1 = \pm 0^\circ{\cdot}25.$$

From (2) we have $r = 38^\circ\ 53'{\cdot}5$,

and by (2′) $dr = 0{\cdot}577\,da + 0{\cdot}770\,d\mathrm{H}$, $d\mathrm{H}$ being the error in the altitude of the measured chord, or the chord's altitude or depression, reckoned from the horizon.

Putting $da = d\mathrm{H} = \pm 15' = \pm 0^\circ{\cdot}25$, we get—

$$dr = \pm 0^\circ{\cdot}24.$$

From (3) we have $r = 41^\circ\ 8'$,

and by (3′) $dr = 2{\cdot}315\,da - 3{\cdot}074\,dh$.

Putting $da = dh = \pm 0^\circ{\cdot}25$, we get—

$$dr = \pm 0^\circ{\cdot}97.$$

Taking the weights inversely as the squares of the probable errors, we find that the results from (1) and (2) have a weight of 15 times that found by (3), and the mean will be—

$$r = 38^\circ\ 38' \pm 6'{\cdot}4.$$

From this mean we find that h should have been $38^\circ\ 38' - 31^\circ\ 12'$, or $7^\circ\ 26'$ instead of 7°, or somewhat greater than measured, as supposed in my note-book. Computing from (2) we find that we should have calculated with $\mathrm{H} = 30^\circ\ 51'$ instead of $31^\circ\ 12'$, or the chord has been measured in a level 21′ lower than the horizon, which is highly probable with the fog spreading over the surface of the sea. The measured chord being too great, and the measured height too small, it follows from (3′) that (3) must give the radius by far too large.

The next occasion I had to observe the fog bow was in 1878, on the North Atlantic Expedition, when returning from Spitzbergen. During August 30, our ship, the *Vöringen*, had a rather tedious work in advancing southwards, on account of the foggy weather prevailing the whole day. In the afternoon we had advanced so far south of Bodö as to approach the Sandhorn, a mountain about 3000 feet high, lying to the east of the route.

At 5h. 20m. p.m. I saw an anthelic fog bow, white, with the outer edge reddish, the inner edge bluish. I measured, with the sextant, the amplitude along the horizon at 76°, the sun's altitude at 12°, and the breadth of the bow at 2°. The temperature of the air was about 14° C. The latitude was about 67° 10′.

Assuming the measured chord to lie in the true horizon, we get by (2) from $a = 38^\circ$, $\mathrm{H} = 12^\circ$, $r = 39^\circ\ 35'{\cdot}5$. But it is highly probable, that the measured chord lay deeper than the

© 1888 Nature Publishing Group

fog-veiled horizon, perhaps some degrees deeper, and it may be quite as possible that I have measured the diameter as a chord. This supposition gives $r = \frac{1}{2}\,76^\circ$ or 38°. The mean of these two determinations is $38^\circ\ 48'$, with a probable error surely not less than $\pm 48'$, or half the difference.

At 6h. 1m. p.m., the circumstances were more favourable. I measured $2a = 76^\circ\ 11'$, $H = 8^\circ\ 11'$, the breadth of the bow 2°. The latitude was 67° 7′. From these data we have, by (2) $r = 38^\circ\ 50'$. Assuming the observed amplitude to have been the diameter, which is very probable, we have $r = 38^\circ\ 5'\cdot 5$. The mean value is $38^\circ\ 28' \pm 22'$.

If the fog bow, like the rainbow, has always the same diameter, we can join the three values thus found for the radius into a mean result. We have thus, giving the single determinations a weight inversely as the squares of their probable errors—

1875 August 7, 2h. 46m. $r = 38^\circ\ 38' \pm 6'$
1878 August 30, 5h. 20m. $r = 38^\circ\ 48' \pm 48'$
1878 August 30, 6h. 1m. $r = 38^\circ\ 28' \pm 22'$

Mean ... ... $r = 38^\circ\ 38' \pm 1'\cdot 4$

The breadth of the bow being 2°, with a probable error of $\frac{1}{20}$ or $\pm 6'$, we get—

for the outermost red ring $r = 39^\circ\ 38' \pm 6'\cdot 2$
for the innermost blue ring $r = 37^\circ\ 38' \pm 6'\cdot 2$.

At 7h. p.m. the bow stood white against the blue sky, the Sandhorn below it. At 6h. 40m. p.m., and sometimes before, I remarked that my own shadow was visible on the fog wall. In order to get a wider view of the phenomenon, I went up upon the roof of the chart-house, where my eye was 27·5 feet (8·4m.) above the surface of the sea. From here I saw how my shadow distinctly imitated all my movements. The shadow of my head appeared dark on a lighter white ground, and from a certain distance surrounded by a concentric coloured glory, in which the colours were arranged in the order of the spectrum, so that the outermost circumference was red, the middle yellow, and the innermost blue. There was no white band in the glory. With the sextant I measured the radius of the yellow ring, which was the most intense, at 1° 31′. The intensity of the other coloured rings was too feeble to allow their radius to be measured with the sextant. From a comparison with the radius of the yellow ring I judged that of the blue at 1° 15′, and that of the red at 1° 45′, with a possible error on both sides of ± 5′. This phenomenon is Ulloa's Ring.

Taking all my results together, we have the following synoptical table:—

Ulloa's Ring.	Radius of inner	blue	1° 15′ ± 5.
,, ,,	,, ,,	yellow	1° 31′ ± 2′.
,, ,,	,, ,,	outer red	1° 45′ ± 5′.
Fog Bow	,, ,,	inner blue	37° 38′ ± 6′.
,, ,,	,, ,,	middle	38° 38′ ± 1′.
,, ,,	,, ,,	outer red	39° 38′ ± 6′.

The fog bow cannot be the rainbow with three or four inner reflections, as these rainbows, if visible, would not be anthelic, but have, for the red rays, distances from the sun of 42° and 43°. Moreover, the intensity of the fog bow is too considerable to be the result of so many reflections in drops of rain. The ordinary or first rainbow, with one inner reflection, has a radius or distance from the anthelic point of 42° 30′ for the red, and 40° 30′ for the violet rays, which gives, the sun's radius being 16′, its innermost radius like 40° 14′. The outermost red ring of the fog bow has a radius of 39° 38′. Its distance from the ordinary rainbow is consequently only 36′. This space we see sometimes covered by the supernumerary rainbows, caused, as Sir G. B. Airy's investigations have shown, by the interference of the rays leaving the raindrop.

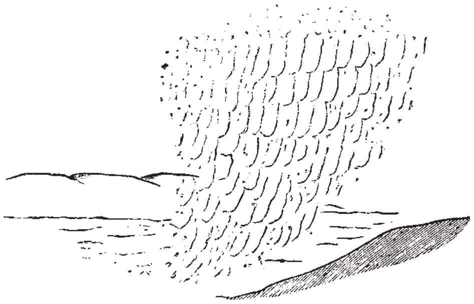
It seems probable that the smallness of the fog globules, as contrasted by the larger size of the raindrops, must enter as an essential part in the explication of the fog bow. In Günther's "Lehrbuch der Geophysik und physikalischen Geographie," ii. p. 151, he speaks of white rainbows, the description of which agrees with the aspect and position of my fog bows, and for which Bravais has given an explanation ("Sur le Phénomène de l'Arc-en-ciel blanc," *Ann. de Chim et Phys.* [3], vol. xxi. p. 348). Not having Bravais' memoir at hand, I may only remark that, as far as I can see from Günther, he assumes the fog drops to be hollow, a supposition which is hardly in accordance with modern investigations.

When I saw the fog bow, I had, I am sorry to say, no polariscope, so that I was unable to investigate the polarization of its light, so characteristic for the rainbow. H. MOHN.

Christiania, January 31.

The Shadow of a Mist.

THE reticulated rippling shadow of the mist described in Mr. Fawcett's letter (NATURE, January 5, p. 224) reminds me of a somewhat parallel phenomenon seen by me a few years ago. I saw a snow-storm some miles away crossing the valley between the Mendips and the Quantocks. It hung like a long, heavy curtain partially obscuring the *bright* western sky. The light shining through the shower showed a fairly regular pattern. On a reddish-brown ground the darker, because denser, parts of the shower took the form indicated roughly by the accompanying diagram. Was the snow falling in spiral streams, and would a similar explanation apply to the shadow of the mist seen by Mr. Fawcett? HENRY BERNARD.



The English Church, Moscow, January 31.

Instability of Freshly Magnetized Needles.

I MADE no attempt to investigate the fluctuations of the dipping needle. They seemed to me to pass away after a few minutes, and I therefore took that method to get rid of them, supposing that the phenomenon was well known to other observers. The variations that I observed amounted to three or four minutes, I should think. It is true that the dip circle which I used was of an ancient pattern; as Prof. Rücker says, hardly up to modern requirements. I did not send it back to the maker for adjustment, as Mr. Whipple says he would have done, because it was lent to me, and it was the best I was able to get.

Recognizing the fact that we could not expect to get the best results from our outfit, it was deemed best to make only one set of observations at each station, and multiply the number of stations as greatly as possible. This made it necessary to do the work quickly at some stations in order to adapt our time to that of trains, or in order to get the drudgery involved in camp-life done within the twenty-four hours. It is probable that at some stations we overdid the matter, and that the observations would have been better if more time had been taken. The dip observations I always regarded as least satisfactory. But all of the work has been published in such a way that its value can be estimated by anyone interested, and everyone is welcome to place whatever value he pleases upon it. We did the best we could under the circumstances, and the expense was met by private means.

The dip circle was returned to Washington when we were done with it, so that I am not now in a position to throw any light upon the subject under discussion. For most of the stations at which observations were made, I think the magnetic elements were determined with as great precision as a single observation would give them, and they seem to me to be as valuable as they profess to be, and not very much more. The fact that so little magnetic work had been done in the central part of the United States seemed to me to justify the plan of making the number of stations large, rather than of trying to attain the precision of observatory methods in field work at a few stations.

FRANCIS E. NIPHER.

© 1888 Nature Publishing Group